retinal receiving area. It is, however, a noteworthy circumstance that these natives are able to pass from the bright tropical glare outside their dwellings to the dark interiors, and *vice versâ*, without showing the temporary derangement of vision which the white man experiences whilst the iris is adapting itself to the new condition. H. B. GUPPY

17, Wood Lane, Falmouth, March 30

Mr. Lowne on the Morphology of Insects' Eyes

IN reference to the discussion between Dr. Sydney Hickson and Mr. Benjamin Lowne, I beg to state that I have been favoured by both of those gentlemen with opportunities of carefully studying their preparations, and I feel it to be my duty to state that in my judgment Mr. Lowne's preparations do not justify the conclusions which he has based on them, and are, in fact, not made with that skill and knowledge of modern histological method which is necessary in order that trustworthy conclusions may be obtained. On the other hand, Dr. Hickson's preparations are thoroughly satisfactory as examples of histological manipulation. Dr. Hickson supports the accepted view as to the termination of the optic nerve-fibres in the nerve-end cells of the retinulæ. Mr. Lowne denies this connection. I have no doubt that such a connection cannot be readily observed in Mr. Lowne's preparations. At the same time I have no doubt whatever that this is because the preparations are badly made. Mr. Lowne's preparations fail to show many other simple features in the structure of the insect's eye, which are readily seen in preparations made by the application of methods now recognised and approved, but not made use of by Mr. Lowne.

I am sorry to see the resources of the Linnean Society employed in publishing a memoir the conclusions of which, although startling in their novelty, are undeniably based upon the mistaken interpretation of defective preparations.

I think it is important that the Fellows of the Linnean Society should know whether the memoir now published is the same which was read a year or two ago at the Royal Society, and whether the Council of the Royal Society took any steps to ascertain the value of Mr. Lowne's preparations, or came to any decision as to the fitness of Mr. Lowne's paper for publication.

March 14 E. RAY LANKESTER

On the Terminology of the Mathematical Theory of Elasticity

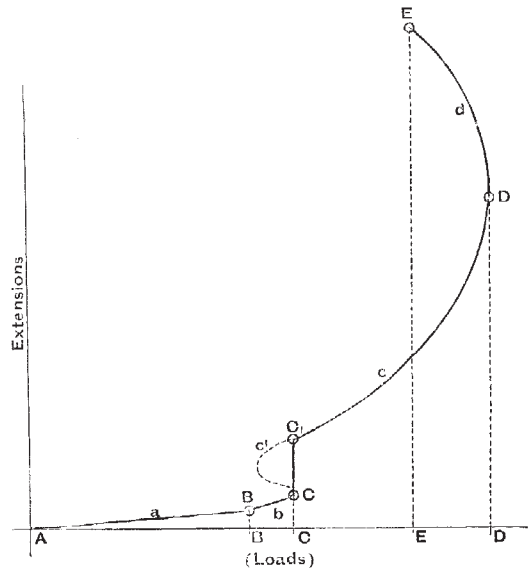
ENGINEERS quite as much as "elasticians" have reason to want some such terminology as that sought by Prof. Pearson (NATURE, vol. xxxi. p. 456), and have equal reason to be indebted to him for undertaking the work which he has at present in hand, which seems already to have given results of practical value as great as their scientific interest.

As I have for some years made a study of the physical side of the problems mentioned by him, I should be glad to make some suggestions as to terminology as contributions to the discussion of the subject in your columns. I will confine what I have to say to what may be called ductile materials (such as wrought iron, ordinary steel, copper, &c), because in these only the whole phenomena are visible. The behaviour of such material in tension is illustrated by the accompanying figure, in which stresses are measured along the horizontal, and strains along the vertical axis.

It is extremely rare to obtain a piece of raw material already in a *state of ease*. *Wire*, of course, is highly strained by its process of manufacture, but that even ordinary bar and plate is also slightly strained, is shown in the manner mentioned by Prof. Pearson. Such initial strains as become visible as *set* by the first stretching up to any load (within limit of elasticity) disappear after one or two applications of that load. The material is then in a state of ease up to that load, but higher loads (still within the limit), on their first application, generally produce more set—the state of ease thus extending only to the stress employed to produce it. The *sets* are, along with the elastic strain, proportional to the stress, their effect being simply to lower the modulus of elasticity. Probably the process of *annealing* will bring the material into a state of ease for all loads at which such a state is possible. I propose to examine this matter further by aid, if possible, of the apparatus described by Prof. D. E. Hughes in the Inst. M. Eng. *Proc.*, 1883, p. 73. In the figure, *a* represents this *condition of perfect elasticity* (maximum state o case being presupposed) and B, the superior limit of this condition, is the mathematical *limit of perfect elasticity*.

After B comes a stage *b*, within which the set is *not* proportional to the stress, although it still remains small; the total extension, therefore, increases faster than the stress. Occasionally this stage does not occur at all, and both its higher and lower limits seem—more than any other points in the life of the material—to be susceptible of change depending on manipulation. Accidental shock will shorten the stage considerably; very gradual loading extends it somewhat. For these and other reasons I therefore suggest that this stage be called the *condition of instability*, or of *unstable equilibrium*.

This condition terminates at C, in what I have called a "breaking-down" in the paper referred to by Prof. Pearson, in which paper I believe the phenomenon was described for the first time. This point is the one called by engineers the limit of elasticity, because it is the only one markedly visible without special apparatus. (The extension at B, on a length of 10 inches, may be about 0·01 inch; at C 0·03 inch and at C_1, same stress, it increases to 0·20, 0·25, and even occasionally 0·4 inch.) If "breaking-down point" be too crude a name, I would suggest *limit of stability*. It should be noted that the stress at this



point does not remain constant, but in reality appears to diminish as the extension goes on, as shown at *c'* (this dotted curve not drawn to scale), a matter on which I am at present experimenting. I should add that, during the application of load at this point, extension appears to be occurring at different parts of the length *successively*, and not at all parts simultaneously, as during conditions *a* and *c*.

In the next stage, C to D, the whole strains consist of a very small elastic portion (apparently closely following the modulus), and a very large set, increasing much faster than the stress. The test bar remains at each load practically constant in its cross-section at all points of its length, and rises in temperature instead of (as in condition *a*) cooling. I would suggest for this stage the name *condition of uniform flow*, the physical applicability of which will be obvious to any one who has seen ductile metal in this condition.

At some point, D, a maximum load is reached, and at *about* the same point (generally, I think, a little earlier, but the difference is small, and not very easy to get at with certainty) the metal begins to flow *locally*, a part becoming much more reduced in cross-section than the rest, and eventually fracture occurs at this place under a less load than D, but with a greater extension, as at E. This final stage, *d*, might be called *condition of local flow*. The loads D and E (as Prof. Pearson suggests) would be *maximum* and *terminal* loads respectively). (Their difference was first pointed out, I think, by Mr. Daniel Adamson's experiments, *Journal* I. and S. Inst., 1878). The maximum *intensity* of stress

occurs always, I think, at E, the cross-section of the bar being proportionately more reduced than the load.

ALEX. B. W. KENNEDY

University College, March 23

The Colours of Arctic Animals

THE white colour of Arctic mammals and birds has hitherto been generally ascribed by evolutionists to protective resemblance, the adaptation to a snow-covered country being attributed to the preservation of individuals which by assimilating to their environment in colour, either escaped detection by their foes, or, on the other hand, were by this means enabled to secure their prey more advantageously. Although a certain weight may, in the case of some species, be fairly given to these organic factors, it always appeared to me that this explanation was not in itself sufficient, in face of the consideration that many of the species so coloured could hardly be said to require such protection on account of persecution, or to derive any obvious advantage therefrom for predatory purposes. A more satisfactory explanation seemed to be that the mode of coloration in question had, at any rate in the first instance, been brought about by natural selection through physical rather than through organic agencies. It is well known that white, as the worst absorber, is also the worst radiator of all forms of radiant energy, so that warm-blooded creatures thus clad would be better enabled to withstand the severity of an Arctic climate—the loss of heat by radiation might, in fact, be expected to be less rapid than if the hairs or feathers were of a darker colour.[1] According to a paper recently published by Lord Walsingham,[2] it seems that this view was entertained as far back as 1846 by Craven,[3] the only addition to the theory required by modern evolution being that we must regard the white covering as having been acquired by the ordinary Darwinian process of the survival of the fittest, *i.e.* by the climatic selection of those individuals best fitted to withstand the extremely low temperatures of their habitat.

It is perfectly familiar to zoologists that most animals occasionally give rise to white varieties, so that the basic variations necessary for the establishment of the required modification in the colour of the hair and feathers would not have been wanting during the gradual approach of the Glacial Epoch. It may be conjectured whether white may not have been the prevailing colour among all warm-blooded animals during this period, with the exception, perhaps, of those species in which the severity of the climate may have been met by an equally effective thickening of the fur. Certain species which, like the stoat and ptarmigan, become white during winter, may, from this point of view, be regarded as reverting seasonally to the mode of coloration which in their ancestors was normal during the Glacial Epoch, the reversion being in these cases brought about by the same influences which formerly fixed white as the most advantageous form of covering. In accordance with this view, it is sometimes asserted that the stoat does not commonly turn white during winter in the south of England, excepting in very severe seasons.[4] Further observations on this point are much needed.

In striking contrast to the white covering of Arctic and Alpine mammals and birds, it has been found that there is a quite opposite tendency for the insects to become darker and more suffused, this melanism being especially noticeable among many of the Lepidoptera. Although numerous speculations as to the cause of this phenomenon have from time to time been advanced, it is in the paper by Lord Walsingham above referred to that what appears to be a true cause has for the first time been suggested. The author has, in fact, most ingeniously extended the very argument which had been adduced to account for the white colour of the mammals and birds to explain the quite opposite melanism of the insects. According to the present view the melanic tendency of northern Lepidoptera must be ascribed to the natural selection of the darker forms owing to the advantage which these would possess in being able to absorb more of the solar radiation than their lighter congeners. The same action must be regarded as here bringing about opposite effects: in the case of warm-blooded animals the loss of heat by radiation is retarded by the white covering, whilst in insects, which develop but little heat by respiration, it is of the utmost importance to utilise as much as possible of the solar energy. This will be seen to be all the more necessary when it is considered that, under Arctic conditions, the solar rays have but little power, and that the pairing of the insects has to be effected with great rapidity. In order to test these views experimentally, the author exposed numerous species of Lepidoptera of various colours to the sun's rays on a surface of snow, and observed the rate at which the insects sank beneath the surface. As might have been anticipated, the darker insects, like *Tæniocampa cærophyllata*, sank more rapidly than white moths like *Acidalia immutata*, which made but little impression on the snow.

The questions raised by these suggestions and observations certainly appear to be well worthy of consideration when discussing the subject of animal coloration. Thus the explanation of the melanism of Arctic insects now advanced may perhaps, when more fully elaborated, throw further light upon the theory of seasonal dimorphism first proposed by Weismann.[1] If, in accordance with the views of this author, we regard the present winter forms of these seasonally dimorphic Lepidoptera as the ancestral Glacial types, it becomes clear why in such white species as *Pieris napi*, the parent Glacial form *Bryoniæ* should be the darker. In the case of *Araschnia levana* the theory does not at first sight apply, inasmuch as the winter form is lighter than the summer generation (*Prorsa*); here, however, both forms are coloured, and there would be but little difference in their relative heat-absorbing powers. The same remark may apply in the case of our own seasonally dimorphic species of *Selenia* and *Ephyra*.

R. MELDOLA

[1] *Trans. Essex Field Club*, vol. i. *Proc.*, March 20, 1880, p. vi.

[2] "On some probable causes of a tendency to melanic variation in Lepidoptera of high latitudes;" the Annual Presidential Address to the Yorkshire Naturalists' Union, Doncaster, March 3, 1885.

[3] "Recreations in Shooting," p. 101.

[4] R. M. Christy in *Trans. Essex Field Club*, vol. i. p. 67.

An Error in Ganot's "Physics"

IN your issue of February 19 (p. 361), E. Douglas Archibald calls attention to a typical error in Ganot's "Physics," 10th edition, p. 325, and assumed that it had run through the ten editions. If he had taken the pains to look back to previous editions the formula would have appeared right, viz.:—

$$P = \frac{0{\cdot}31\ V(H - \frac{5}{8}FE)}{(1 + \alpha t)\,760}.$$

In going over the text of earlier issues of the book some minor errors are discoverable, but do not detract materially from the value of the same to the careful student

FRANK E EMERY,
1st Asst. Sci. Dept.

Mountainville, Orange Co., New York, March 4

WITH reference to the letter of Mr. Frank E. Emery on mine, calling attention to the typical error in Ganot's "Physics," I beg to say that though in some of the earlier editions the error may not exist, it occurs in the 5th and 10th, both of which are in my possession. The inference is very strong that if it occurs in these two it occurs in the editions *intervening*, and thus in HALF of the editions published. The first five editions are now getting out of date, so it is not of much value to people if the error does not exist in them.

I would also observe that if Mr. Emery takes the pains of reading my letter over again he will notice it was explicitly stated to be for the benefit of the large class of students who are *not careful*.

My purpose was in no way to run down Ganot, but to warn people of a pitfall in it.

E. DOUGLAS ARCHIBALD

Tunbridge Wells, March 23

Exceptional Whiteness in Tropical Man

SINGULARLY enough, being encamped in the same place as that from which the paper on "The Blackness of Tropical Man" was written to NATURE some months ago, the converse, a case of the whiteness of this class of man, presented itself unexpectedly. While entering, to-day, the native village of Jeykondasholapurm, that had sunk to nothing from having been the capital of a native dynasty in the south of India, and situated about lat 11° N. and long. 78° E., the writer observed an apparently white woman sitting on a doorstep by the side of the road, with flaxen-coloured

[1] I may take the present opportunity of pointing out to those who possess the English edition of the "Studies in the Theory of Descent" that an error inadvertently occurs in the numbering of the figures in Plate I. Figs. 2, 3, 4, and 5 should have been numbered respectively 3, 5, 2, and 4. I am indebted to Mr. E. B. Poulton for kindly calling my attention to this transposition.